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Expansions in paid parental leave and mothers' economic progress

Gozde Corekcioglu [a], Marco Francesconi [b,*], Astrid Kunze [c]

[a] Ozyegin University, Nisantepe Mah. Orman Sk. 34794 Cekmekoy Istanbul, Turkey
[b] University of Essex, Wivenhoe Park, Colchester CO4 3SQ, United Kingdom
[c] Norwegian School of Economics, Helleveien 30, N-5045 Bergen, Norway

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ABSTRACT

This paper investigates the impact of reforms extending paid parental leave on mothers' progress to the upper echelons of their companies. Using employer–employee matched data and examining a series of reforms between 1987 and 2005 in Norway, we find that longer parental leave neither helped nor hurt mothers' chances to be at the top of their companies' pay ranking or in the C-suite up to 25 years after childbirth. This holds true also for highly educated women and high performers across all sectors. Key career determinants, such as hours worked and promotions, are unaffected in the short and long run. Finally, fathers' career progression and within-household gender wage gaps have also remained unaltered.

1. Introduction

Despite considerable progress towards gender equality in education and labor market outcomes since the 1960s, much research documents the persistent under-representation of women among top earners (Albrecht et al., 2003, 2018; Arulampalam et al., 2007; Goldin, 2014; Keller et al., 2020) as well as in leadership positions (Miller, 2018; World Economic Forum, 2020). Besides fairness considerations, the lack of gender diversity at the top may be inefficient (Bertrand and Duflo, 2017). This is important since top earners and top executives are powerful economic players. Moreover, a large share of the best talents in any economy is likely to be reflected in top earnings (Hsieh et al., 2019; Guvenen et al., 2020).

The two main factors that account for a persistent share of gender gaps in careers and pay are women's greater career discontinuities and shorter work hours, both of which are strongly associated with motherhood (Bertrand et al., 2010; Angelov et al., 2016; Antecol et al., 2018; Bütikofer et al., 2018; Kleven et al., 2019; Iversen et al., 2020; Keloharju et al., 2022).[1] Most industrialized countries — with the notable exception of the United States — have introduced and expanded nationwide government-funded parental leave programs with the explicit intent of reducing the cost of having children, enabling mothers to combine career

* Corresponding author.
E-mail addresses: gozde.corekcioglu@ozyegin.edu.tr (G. Corekcioglu), mfranc@essex.ac.uk (M. Francesconi), Astrid.Kunze@nhh.no (A. Kunze).

[1] Azmat and Ferrer (2017), however, find no evidence of a motherhood penalty among associate lawyers in the United States. An alternative approach to interpret this penalty emphasizes the importance of discrimination. Lang and Kahn-Lang Spitzer (2020) provide an up-to-date review of this literature. Testing this interpretation directly is beyond the scope of our paper.

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and family, and leveling the playing field between men and women in the labor market (Olivetti and Petrongolo, 2017; Rossin-Slater, 2018).

In this paper, we first document the evolution of the share of women in the upper decile of their firms' earnings distribution and in leadership positions (i.e., CEO, CFO, and board directors, which we refer to as the "C-suite" of a company). Using high-quality Norwegian employer–employee matched register data from 1983 to 2013, we show that all women have made a steady, but nonetheless slow, progress in accessing top earnings jobs, from about 15% to 33%. Mothers' representation has increased more, but from a lower base, going from 10% at the start of the period to approximately 28% at the end. Top-pay jobs are still overwhelmingly occupied by men. Similar results emerge when it comes to holding positions in the C-suite.

We then ask whether eight stepwise expansions of job-protected paid parental leave from 18 weeks in 1986 to more than a year by 2005, including the introduction of earmarked leave for fathers since 1993, have helped or hurt women reaching top positions. We document that the parental leave reforms had no effects on fertility and employment, and most mothers, even among high earners, use leave fully. Using a regression discontinuity design, in which mothers who qualified for the leave extension are compared to mothers who just missed the opportunity of a longer leave, we find that each of the expansions had a statistically insignificant impact on the probability that mothers reach the upper decile of the pay distribution within their companies. The effects are zero in the short, medium, or long run. Similarly, the expansions had no effect on the likelihood that mothers climb to the C-suite or occupy managerial positions in their firms. These null results hold even when we estimate the cumulative effect of the incremental leave duration expansions; which did not set aside any explicit paternity leave. They also hold if we combine each reform-specific sample into one larger sample, which gives us further statistical power.

Parental leave in Norway is paid at the pre-birth wage for eligible mothers, who have worked at least 6 out of 10 months before birth. The wage replacement is funded through general taxation. After exhausting her leave, a mother can return to work for the same employer in the same (or similar) job as she had before childbirth due to job-protection. In the short run, theory predicts that, if firms do not bear any cost and the leave expansions are financed by non-distortionary taxes, both labor supply and labor demand remain unaltered and we expect to observe no change in wages. Due to the extension of leave duration, some workers, especially those with high skills (and more likely to reach top positions), who would have returned early are now incentivized to return later. This may curtail mothers' economic success by reducing the accumulation of their human capital in both the short and the long run.

In imperfectly competitive labor markets, where skills are not easily substitutable and workers have limited mobility (Manning, 2003; Le Barbanchon et al., 2021), demand changes may be important for high-achieving mothers. These women, in fact, are likely to fall behind on the career ladder in the absence of an extended job-protected leave, as they tend to change firm and be more vulnerable to career interruptions with greater human capital depreciation and atrophy of skills while out of work (Demougin and Siow, 1994; Anderson et al., 2002; Ejrnæs and Kunze, 2013; Adda et al., 2017). This would lead to a wage reduction, in general, and to a lower likelihood to reach the upper decile of the within-firm pay distribution for each mother, in particular.

If employers, however, value continuity in employment relationships with their high-skill workers, high-achieving mothers could face a steep positive pay profile after reentry and, in the longer run, climb the job ladder to the top of their firms' pay distribution. Long generous leave may then facilitate the "rebound" effect of female earnings, first documented by Mincer and Ofek (1982) in the context of any withdrawal from the labor market, and eventually lead to substantial wage growth over highly skilled work-oriented mothers' careers.[2] With ambiguous theoretical predictions about the impact of the extension of government-provided paid maternity leave on high-achieving mothers' careers, the question remains essentially empirical.

In this study we focus on whether mothers reach a position in the company's top earnings decile and the C-suite, both of which are long-term outcomes. Few studies have analyzed similar outcomes, perhaps due to data limitations.[3] An exception is the work by Corekcioglu et al. (2020), which reports evidence of nonpositive effects of the 1993 reform in Norway on mothers' probability of being in the top decile or quartile of a firm's earnings distribution in selected years after childbirth. In the present study, we go well beyond our previous work and existing studies. In particular, we present detailed evidence from a more comprehensive analysis over several reforms. We also include detailed examinations of a direct measure of reaching a powerful position — the C-suite — as well as father's outcomes, within household pay gaps, and the pooled and cumulative impact of all the pre-1993 interventions, as well as the 2005 reform. Furthermore, we broaden the economic analysis by investigating mechanisms, and possibly unveiling why we find zero effects through impacts on the main determinants of careers.

Much of the existing empirical research on the impact of parental leave on labor market outcomes focuses on average wages, wage growth, and employment. Evidence on the sign and significance of the long-term effects tends to be mixed but with a convergence towards small or zero effects, across several countries, including those with government mandated provisions and large register data. In a study for West-Germany leveraging five reforms enacting substantial leave duration extensions, Schönberg and Ludsteck (2014) find evidence of a small negative effect in the short term, and an overall small, statistically insignificant, impact on maternal employment, labor income, and the probability of moving out of full-time work up to the child's sixth birthday. Lalive et al. (2014) detect no detrimental effect on mothers' earnings in the first five years after childbirth using three major reforms in Austria. Analyzing the same three reforms, Kleven et al. (2024) find short-term sizeable negative (positive) impacts of leave extensions

[2] In anticipation of future female hires being on leave for an extended period of time, employers could respond to extended leave entitlements by statistically discriminating in their hiring strategy of, or wage offers to, childless young women. This is an important margin, which is left for future analysis.

[3] Although not at the core of their analysis, Kleven et al. (2024) show that a series of Austrian reforms had no long-term effects among mothers in the top quartile of the pre-birth earnings distribution in the economy.

(reductions), which vanish however three years after childbirth.[4] Importantly, most of these reforms involved substantial leave duration with relatively low wage replacements. Although the evidence so far suggests zero effects at the mean, there might be asymmetries across the income distribution, with non-zero impacts for mothers at each end of the spectrum, especially at the top. Exploring this possibility is one of the contributions of our paper.

For Norway, which combines long leave duration *and* full (or high) wage replacement, Dahl et al. (2016) show that the parental leave reforms from mid 1980s to 1992 had small positive, but statistically insignificant, effects on mother's annuity income up to 14 years after childbirth. Their focus, therefore, is on reforms enacted before the introduction of a full year of leave and before the paternity quota. The existing evidence of the effect of the 1993 parental leave on fathers' labor market income is limited to short-run effects and provides mixed results. Rege and Solli (2013) find a negative impact up to the child's fifth birthday, whereas Cools et al. (2015) report a null effect.[5] We extend these earlier contributions by focusing on within-firm pay rankings as well as executive positions, looking at a larger raft of reforms of paid leave, and considering the pay gap within households.

Earlier studies find some evidence of effect heterogeneity across subgroups of women. For instance, Anderson et al. (2002) and Ejrnæs and Kunze (2013) detect relatively larger negative effects for high skilled mothers. We add to this evidence by considering potential heterogeneous impacts by child parity, mother's education and industry of employment. At higher parities, women may have greater family commitments and lower career prospects. On the other hand, college educated mothers and those employed in specific industries (e.g., the finance industry) may be arguably better placed to climb to highly paid jobs and the C-suite of their organizations.

Furthermore, we broaden the perspective of the existing literature by presenting a systematic analysis of channels, which are related to human capital and job mobility, such as work experience, firm tenure, hours of work, internal promotions, and firm-to-firm transitions. An interesting question is to see whether some of these channels are differently affected than others by the parental leave reforms in a way that can explain the zero effect results. Although the short-run impact is ambiguous, there might be good reasons for a positive rebound effect in the longer run. We document that the causal effects are zero throughout the entire post-birth period across each of the channels considered.

In addition to contributing to the literature on parental leave and long-term labor market outcomes, we also contribute to the burgeoning literature on gender diversity. We document a slow decline in the under-representation of mothers (and women, more generally) at the top echelon of their companies across the entire Norwegian economy. The literature so far has focused on convenience samples (Hoogendoorn et al., 2013) or specific sectors or organizations (Auriol et al., 2022; Hospido et al., 2022).

2. Institutional background

Norway has a long history in supporting working women around motherhood, which dates back to the late nineteenth century and has continued through to the post-WWII period (Vollset, 2011; Ellingsæter et al., 2020).[6] In 1956, women became eligible to maternity compensation through the sickness benefit scheme that replaced part of pre-birth earnings and provided protected leave for up to 12 weeks (Utredninger, 1996, p.214). With the 1978 Social Insurance Act reform, paid parental leave was granted for 18 weeks.

Eligibility required mothers to have worked six of the 10 months before birth and earn more than the basic income. Mothers were entitled to a minimum of six weeks after birth. Although there was no explicit formal paternal quota, both parents could share the remaining 12 weeks, but mothers typically took the whole leave available.[7] The mandates provided 100% income replacement up to a generous earnings threshold (equivalent to six times the basic income).[8] Employers could not dismiss workers for taking leave, and parents had the right to return to the same (or comparable) job. These features remain in place over the entire observation period that we study.

From 1987 to 1993, Norway introduced a series of seven reforms that expanded paid parental leave from 18 weeks to 42 weeks at 100% income replacement (see Table 1). In 1985, the Labour Party led by Gro Harlem Brundtland won the general election with a strong mandate to reform. One of the main goals of Brundtland's government in power since 1986 was to achieve greater gender equality, starting from her own cabinet in which eight of the 18 ministers were women. The extension of the mandated paid parental leave was a key part of this program. Besides political feasibility, the staggered introduction of the annual extensions might have been due to the 1985 oil price shock which led to an unexpected public deficit and a significant devaluation of the Norwegian krone. This was then compounded by the banking crisis, which began biting in 1988 and continued through to 1992.

[4] Other studies on the effect of parental leave on mother's earnings include Baum (2003), Ejrnæs and Kunze (2013), and Bailey et al. (2019). The first study finds a small and statistically insignificant effect of the 1993 US Family and Medical Leave Act on wages. The latter two papers identify significantly negative impacts on wages of mothers in Germany and California, respectively. Examining the California's paid family leave program, Rossin-Slater et al. (2013) find that longer leave use is associated with greater weekly work hours and wage incomes of employed mothers. Looking at the same reform, Baum and Ruhm (2016) confirm the positive impact on employment probabilities for mothers nine to 12 months after birth, especially among those with relatively weak labor force attachments.

[5] Ekberg et al. (2013) and Patnaik (2019) also find no effects of paternity quotas on fathers' labor market outcomes in Sweden and Quebec, respectively.

[6] Most of these early regulations were primarily intended to protect the child and mother's health, especially women in factory jobs. Since 1977, parliamentary debates have regularly given prominence to the goal of gender equality, and in particular in relation to mothers' and fathers' rights.

[7] Working fathers had the right to two weeks of unpaid leave after birth. Their income would have been typically replaced by their employers through bilateral agreements (Work Environment Act, 1 July 1977).

[8] This and all the thresholds related to the subsequent reforms were nonbinding for most mothers. When they were exceeded, employers topped up benefits so that forgone earnings were fully replaced, even for women at the top of the pay distribution (Utredninger, 1996, p.218).

Table 1
Parental leave reforms in Norway, 1978–2005.

Reform date	Total weeks of leave	Income replacement	Maternal Quota	Paternal Quota
01.07.1978	18	100%	6 weeks	
01.05.1987	20	100%	6 weeks	
01.07.1988	22	100%	6 weeks	
01.04.1989	24 (30)	100 (80)%	6 weeks	
01.05.1990	28 (35)	100 (80)%	6 weeks	
01.07.1991	32 (40)	100 (80)%	2 + 6 weeks	
01.04.1992	35 (44.4)	100 (80)%	2 + 6 weeks	
01.04.1993	42 (52)	100 (80)%	3 + 6 weeks	4 weeks
01.07.2005	43 (53)	100 (80)%	3 + 6 weeks	5 weeks

Note: 'Maternal Quota' refers to the leave reserved to mothers, which consists of 6 weeks after childbirth. Since 1991, additional 2 or 3 weeks must be taken before childbirth. 'Paternal Quota' refers to the leave reserved to fathers after childbirth, which consists of 4 weeks after the 1993 reform and 5 weeks after the 2005 reform.

Starting in 1989, parents could also choose between a package comprising shorter leave with full income replacement and a package with longer leave at 80% replacement. The 1993 reform set aside a four week quota of paternity leave for the first time worldwide. This added to the 48 weeks at 80% replacement taken by the mother, leading to a total of 52 weeks at the household level. If a couple opted instead for full income replacement, they would have enjoyed a total of 42 weeks of leave, of which (at least) four had to be used by the father. The last reform we consider was introduced in 2005, in which the father's quota was extended to five weeks, without changing the mandated length of maternity leave.

Because our aim is to identify the impact of the interventions on women's ability to reach top-pay positions, arguably a long term outcome for which we need a long time horizon after childbirth, we do not consider later reforms given our observation window stops in 2013. By including the 1993 and 2005 reforms, we can evaluate the role played by paternity leave on mothers' outcomes (as well as on fathers'), while giving us a total increase of 20 weeks of paid maternity leave, a natural comparison to the initial 18 weeks introduced in 1978.

As our study naturally takes a long term perspective, there might be concerns that other public policies might interact with our treatment. Among such policies, some of the most relevant include the 1998 Cash-for-Care program, the changes in the public child care provision introduced in 2002, and the gender board quota reform passed in 2005 but not fully implemented until 2008. Since each of these policy changes did not occur at the same time as the parental leave reforms under analysis, we ought to emphasize that they do not affect our definition of treatment status and thus cannot contaminate our estimated treatment effects. More details on these other programs are provided in the Online Appendix A.

3. Data

3.1. Data construction

Our analysis uses employer–employee matched panel data extracted from several registers on the entire population of employees and the universe of private- and public-sector firms in Norway. This enables us to have detailed workers' employment and earnings information from 1967 to 2013, and to follow them with their employers from 1983 to 2013. Earnings information is recorded without any top- or bottom-coding. For each firm, we observe the entire population of workers at the establishment, including those at the very top of the organization, so that we can determine the exact rank occupied by each worker in the earnings distribution within her/his company. With unique identifiers for individuals and firms, we can match workers to their employers and partners to one another, and follow them over time until 2013.

Besides earnings and employment, the registers provide information on demographics, education, and, from 1997 onwards, contracted hours worked. Data on all births by month and year over the entire period are obtained from birth registers and merged with detailed individual receipt of parental leave benefits and duration obtained from the social welfare registers. These contain complete records for all mothers and fathers from 1992 onwards.

To analyze individual earnings positions within firms, we focus on annual total earnings, reported in the tax registry and deflated using the consumer price index with 1998 as base year. This measure comprises all types of formal remuneration from work, including overtime pay, performance-related pay and bonuses, and benefit income. We also analyze income adjusted for hours of work, using the full-time equivalent monthly wage measure constructed by Statistics Norway with hours data from wage statistics information.

Our final sample contains all individuals employed and with positive earnings from the first time they entered the labor market after completing education (defined at age 18 for those without a university qualification, and age 24 for those with a college degree or more) up to age 60. To construct a meaningful measure of intra-firm gender composition in top positions, we exclude firms with fewer than 4 employees and the self-employed.[9]

[9] The results in Section 5 are not sensitive even if we select companies with 10 or more employees. Furthermore, to comply with the eligibility criteria imposed by the Norwegian regulations, the sample used in the regression discontinuity analysis is restricted to women who received labor income in the year prior

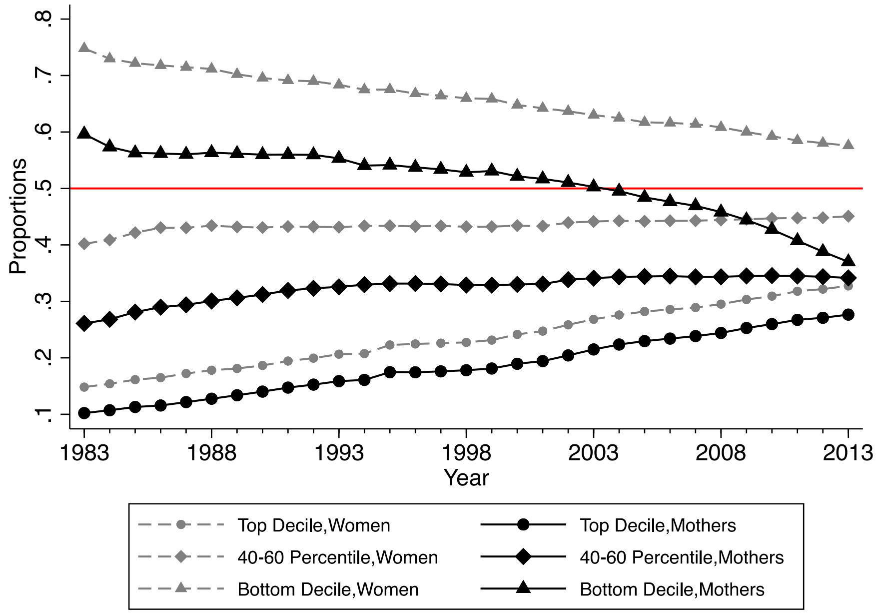

Fig. 1. Proportions of women and mothers in the top, middle and bottom of the within-firm earnings distribution, 1983–2013. *Note*: The sample includes the population of women aged 18–60 with completed education cycles. The horizontal line at 0.5 indicates equality between women (or mothers) and the rest of the population in the same age brackets.

3.2. Outcomes

We use three different measures to identify gender diversity in high-pay jobs and at the top end of the corporate world. The first is an indicator of whether a female employee is in the top decile of her firm's earnings distribution. Different companies could use different pay structures, and women may be at the top in low-paying firms.[10] In spite of this, appearing among the best paid workers in any given organization can be taken as a clear signal of high performance and value to the firm, irrespective of job titles or career tracks. To construct this measure, we exploit that we observe earnings with no differential attrition due to non-response or income top-coding and the universe of employees in all firms in the economy over the sample period.

A slight variant of this measure is redefined with an indicator variable taking value one if a mother is in the top decile of the salary distribution among all women and men in the same age group in their firm, and zero otherwise. We distinguish eight age categories, seven of which are defined in 5-year bands and the first is slightly broader (i.e., 18–24, 25–29, 30–34, 35–39, 40–44, 45–49, 50–54, 55–60). This outcome accounts directly for the fact that it might be difficult for women to be at the top of their organization at the time of childbirth when they are typically young or at the early stages of their careers.

Our second measure is a more direct measure of powerful positions given by specific leadership positions within each company, a proxy for being in the C-suite of the organization. Using data from the job title register, which are available from 2003 onwards, we can identify the chief executive officer, the chief financial officer, all the board directors and the chair of the board for each firm in the data. Our measure takes value one if a woman holds one of these four positions, and zero otherwise. Finally, our third measure, which is based on register-level occupational data, singles out all the employees with managerial responsibilities within an organization from 2010 to the end of the sample period. This variable takes value one if a woman has managerial duties, and zero otherwise. Because this outcome identifies managers across all hierarchical levels and covers a short time horizon over the sample, we focus more on the previous two measures.

Within each household, parental leave may affect the father, either directly (through the leave he actually takes) or indirectly (through the leave taken by his partner). In addition to studying how extensions of parental leave have separately affected mothers and fathers at the top, we also investigate if they improved pay differentials between mothers and men in the same household. The differential between partners is given by $G_{ht} = \log(w^m_{ht}) - \log(w^f_{ht})$, where w^s_{ht} is the pay measure of the female ($s = f$) or male

to childbirth. One could argue that excluding low-earning mothers (i.e., women who might have received little income in the year before birth) and employed in small (low-paying) firms could upward bias our estimates to reach the top decile. To account for this possibility, we repeated our baseline analysis keeping women with lower earnings and in smaller firms in the estimating sample, and setting their outcome measures to zero. All our main results are unaffected.

[10] Our measure, therefore, is robust to differential sorting of mothers into firms both pre- and post-birth based, for instance, on expectations of maternity leave.

Fig. 2. Proportions of women and mothers in executive (2003–2013) and managerial positions (2010–2013). *Note*: See the note to Fig. 1 and the text for more details.

($s = m$) partner in household h for year t. The evidence for Norway on G is surprisingly scant, especially if one takes a long run perspective.[11]

3.3. Evolution of mothers' economic progress

Fig. 1 displays the trends between 1983 and 2013 in the first measure of gender diversity just described, i.e., the proportion of mothers whose annual pay is in top decile of the earnings distribution of their companies. Mothers are defined to be all women with children. Equality is attained at 50%, shown by the horizontal line. To have a more complete picture of the evolution across the entire distribution, the figure also presents the proportions of mothers in the middle of the distribution (40th to 60th percentiles) and in the bottom decile within their firms. For comparison, it also displays the trends for all women.

At the start of the period, mothers (and women in general) were severely under-represented at the top and over-represented at the bottom of the within-firm pay distribution. Over the 31-year period under analysis, however, the fraction of mothers in the top earnings decile has more than doubled, from about 10% in 1983 to nearly 28% in 2013. This is trend is remarkable in parallel to all women though on a lower level. The share at the bottom decile instead has substantially declined, from 60 to 35%. Mothers' representation in the middle of the distribution has gone up, but more modestly than at the top, from one-quarter to one-third over the sample period. Similar trends emerge for the proportion of mothers in the top decile of their age–salary distribution.

Fig. 2 displays the shares of mothers and women in top executive posts. In 2003, only 23% of such posts were occupied by women, and 20% by mothers. By 2013, those figures increased to almost 30 and 37%, respectively. Part of this upward trend is likely to reflect the 2005 gender quota law. If we use the broader measure of management based on the occupational code, we observe 35% of leadership posts occupied by mothers, and slightly more by all women, from 2010 onwards. Although both Figs. 1 and 2 show an ascent of mothers to their firms' top echelon, we do not know if the parental leave reforms played any role, facilitating mothers' return to top-flying careers after childbirth.

Fig. 3 displays the patterns of two measures of G (one based on total annual earnings, the other on full-time equivalent monthly wages) for mothers in the top decile of their firm's pay distribution and for all mothers. For those in the top decile, the intra-household annual pay gap is close to 0 up to the late 1990s and becomes negative since then. From the start of the 21st century, therefore, mothers at the top of their firm's pay distribution have been earning more than their partners, and nearly 10% more by 2013. Adjusting for hours worked increases the pay penalty for mothers, indicating that this group of highly paid women work longer hours than their partners. By the end of the period, the gap is again negative reflecting a pay premium of about 5% for female top earners. The average intra-household pay disadvantage for all mothers, instead, is sizeable and still in excess of 40% in 2013, although declining from 65% since the late 1980s. The penalty in full-time equivalent monthly wages for all mothers has instead remained stable around 20% over the period.

[11] Hart et al. (2022) provide evidence on the relative earnings of couples in response to the extension of paternity leave duration in Norway in 2009. They find no medium-term effects on the mothers', fathers', or relative earnings outcomes of the paternity leave extension.

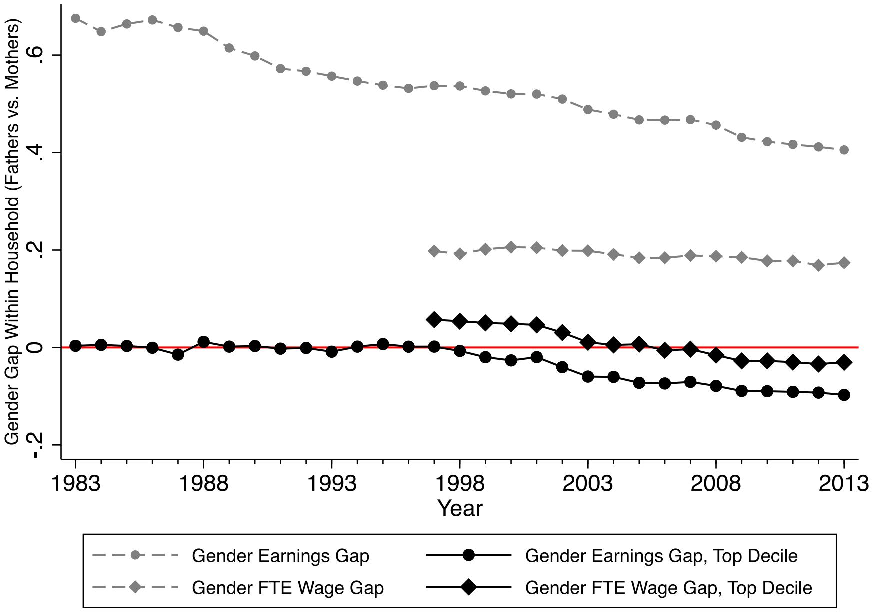

Fig. 3. Gender gaps within the household. *Note*: The figure shows intra-household gaps separately for all mothers and for mothers in the top earnings decile using annual earnings and full-time equivalent wages. 'Gender Gap Within Household' is calculated using annual earnings for all mothers and their partners in a given year. 'Gender FTE Wage Gap' is computed using full-time equivalent wages (available from 1997 onwards). The figures with 'Top Decile' report the corresponding gaps for mothers in the top decile of their firm's earnings distribution and their partners. The horizontal line at 0 indicates where gender equality within the household is achieved.

Table 2
Mean characteristics of women and men in the RD sample around the 1993 reform.

Years since reform	0 Women	0 Men	10 Women	10 Men	20 Women	20 Men
Age (years)	29.97	32.35	40.03	42.21	49.73	51.35
Education (years)	13.52	13.47	13.59	13.51	13.60	13.50
Married or cohabiting (=1 if yes)	0.60	0.60	0.69	0.71	0.64	0.68
Total number of children	1.82	1.94	2.49	2.60	2.55	2.66
Age at first birth	26.41	28.24	26.38	28.20	26.33	27.90
Weekly hours worked			32.55	36.70	33.08	36.31
Years of work experience	9.98	12.50	20.05	22.42	29.78	31.71
Years of firm tenure	3.75	4.08	5.51	6.35	8.90	9.69
Number of jobs held	2.90	2.73	3.05	2.87	3.09	3.00
Annual earnings (in NOK)	176,678	289,568	252,036	416,042	376,022	566,332
Proportion in top earnings decile	0.16	0.35	0.21	0.48	0.24	0.57
Proportion in top earnings-by-age decile	0.14	0.46	0.18	0.55	0.21	0.71
Proportion in executive positions			0.19	0.51	0.30	0.65
Proportion in managerial occupations					0.31	0.55
Annual rate of internal promotions	0.04	0.08				
Number of individuals	32,177	36,853	28,585	33,273	28,890	29,883

Note: Figures refer to all mothers and fathers who had a child in the six months around the April 1993 reform. This corresponds to the analysis sample used for the regression discontinuity estimates (abbreviated as the RD sample) around the 1993 reform. All parents are followed over time. Annual earnings are real and deflated with CPI (1998 = 100). The reported outcome means indicate the mean proportions of mothers and fathers among all employees within the top decile of their firm's earnings distribution or in executive and managerial positions in a given year. The outcome means are computed for all mothers and mothers for the indicated years, rather than just for the RD sample.

3.4. Sample characteristics

Summary statistics of the main variables used in the analysis are presented in Table 2. They refer to all working women and men who had a child in the six months around the 1993 reform.[12] To give an idea of the change over time, we report figures for 1993, 2003, and 2013, that is, when children were 0, 10, and 20 years old respectively, provided individuals were in employment in those years. Selecting adjacent years delivers similar snapshots of the sample.

[12] Summary statistics for parents who had a child around the other reforms provide comparable evidence are not presented for space concerns.

At the start of the period, mothers were nearly 30 years old, and fathers about two years older. By the end of the period, the average age of this cohort of parents was about 50 and 51, respectively. Mothers and fathers had similar schooling levels, with nearly 13.5 years of completed education. For most of them, mothers and fathers alike, the child born in 1993 was the second, and on average they had one additional child by 2013. The fraction of individuals in a marriage or a cohabitation was close to 60% in 1993, and this rose to about two-thirds 20 years later.

Worked hours were stable over time at about 33 and 37 per week for women and men, respectively. At the time of the 1993 birth, mothers had already accumulated almost 10 years of work experience, and fathers nearly 13. By the end of the period, the corresponding figures were 30 and 32 years, respectively. Firm tenure also increased smoothly over time, from approximately four years in 1993 to 9 or 10 years at the end of the panel. This is reflected in the relatively stable mobility across employers, with an average of 3 jobs held between childbirth and 2013.

Over the 20 year period, real annual earnings more than doubled for mothers and rose by 95% among fathers. This is reflected in the increases in the proportion of mothers and fathers in the top earnings decile within their firms, confirming the patterns observed in Fig. 1 for mothers. It is also reflected in the other outcome measures, including the proportion of mothers and fathers in the top earnings-by-age decile.

Finally, we also analyze internal promotions. To do this, we use comprehensive data on over half a million white collar worker–year observations across 4000 plants from 1987 to 1997. The data are merged to the registers from plant-level job surveys compiled by the Confederation of Norwegian Enterprise, the primary employer association in Norway. The sample includes workers at private sector firms and over-represents manufacturing but retains broad coverage.[13] These data contain detailed job information that allows us to assign workers to one of seven hierarchical ranks defined consistently across plants and over time. We can track individual promotions within the same organization as well as promotions that involve a change of employer. Table 2 shows that the annual promotion rate for women was 4%, just half the rate experienced by men.[14]

4. Research design

We use the same identification strategy in each of the eight reforms that took place between 1987 and 2005 and employ a regression discontinuity (RD) design (Hahn et al., 2001).

Let y_a denote the relevant labor market outcome measured at child age a ($a = 1, 2, \dots,$up to age 26 depending on the reform), x be the birth date (year and month) of the child, and t the date of the reform's enactment. For every mother in the data and for each reform, we fit the following regression model[15]:

$$y_a = \alpha_a + f_{\ell,a}(t-x)\mathbb{I}(t-x) + \left[\beta_a + f_{r,a}(x-t)\right]\mathbb{I}(x-t) + \varepsilon_a, \tag{1}$$

where $\mathbb{I}(z)$ is an indicator that takes value one if the event z occurs and zero otherwise, $f_{\ell,a}(\cdot)$ and $f_{r,a}(\cdot)$ are unknown functions of the time distance to and from the reform, respectively, which vary with child age, and ε_a is an error term. The parameter of interest is β_a, which captures the intention to treat (ITT) of the reform offering additional weeks of paid leave on the outcome y_a.

To obtain an average treatment effect, β_a must be adjusted by the change in leave compliance around the cutoff t from another RD regression similar to Eq. (1). This is not possible for the reforms that were introduced before 1992, because take-up information is not available. For the 1992, 1993, and 2005 reforms, for which we have information, we find that all eligible mothers extended their leave duration by the additional number of days allowed by the law, irrespective of whether we consider women in general or those in the top earnings decile (see Appendix Figures C.1 and C.2). This indicates a compliance close to 100%, in terms of both participation and utilization of leave (i.e., extensive and intensive margins, respectively), across all mothers as well as high-achieving mothers who reach the top decile 10 years after childbirth.

The β_a estimate from Eq. (1), therefore, should be close to the average effect on the treated for those three reforms. There is no (anecdotal) evidence that take-up rates were lower for the earlier policy interventions. This is why we focus on the ITT estimates for the rest of the paper.[16]

Estimating Eq. (1) for every post-reform year separately allows us to trace out the life cycle pattern of the ITT effects from a minimum of eight years (after the 2005 reform) to a maximum of 26 years (after the 1987 reform) in the case of the probability of being in the top decile of the within-firm pay distribution. When our measure of gender diversity at the top is the likelihood of

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[13] For more details, see Kunze and Miller (2017).

[14] In the Online Appendix Table B.1, we report the same descriptive statistics for workers in the top decile of their firms earnings distribution and for executives and board members who had a child in the six months around the 1993 reform. Compared to the whole population of parents, top earners were on average slightly older, substantially more educated, more likely to be married or cohabit, had their first child later, accumulated more work experience, and worked in larger firms. Not only were their mean earnings much higher, which is true by construction, but their weekly hours worked were also greater, especially among mothers. While male top earners have similar promotion rates to the whole population of fathers, female top earners have a strikingly higher rate at about 18%. The average characteristics of executives and board members are typically in between those of the whole population and those of top earners, except for firm size, as they worked in smaller organizations on average.

[15] Given time is the running variable with a treatment date as the threshold, our design reflects the regression discontinuity in time approach described by Hausman and Rapson (2018). Notice, however, that our application does not lack cross-sectional variation (as we have the universe of women having a child around the cutoff date) and does not ignore the time-series properties of the data (as we analyze both short- and long-run effects).

[16] For fathers, instead, the take-up rate is lower at the start, about 40% in 1993, but growing considerably over time to about 85% in 1998, and essentially universal after that year. Furthermore, as the difference in compliance rates between treatment and control groups on each side of the cutoff t is never large, scaling up the ITT estimates does not have much scope.

Table 3
Balance tests.

Sample	All women		First-birth		Large firms		Movers	
Outcome	Age	Education	Age	Education	Age	Education	Age	Education
Reform 1987	−0.059	−0.198***	0.048	−0.142	−0.282	−0.217	−0.047	−0.188***
F-test (p-value)	0.00252		0.125		0.0536		0.0065	
Observations	24 528		12 481		9100		22 311	
Reform 1988	−0.050	−0.104	−0.034	−0.069	−0.015	−0.148	−0.000	−0.099
F-test (p-value)	0.0911		0.419		0.159		0.125	
Observations	28 209		14 098		10 377		25 620	
Reform 1989	−0.088	0.012	0.012	0.030	−0.126	−0.060	−0.079	0.009
F-test (p-value)	0.834		0.711		0.561		0.884	
Observations	30 217		14 866		10 622		27 290	
Reform 1990	−0.103	−0.079	0.025	−0.039	−0.260	−0.235**	−0.013	−0.064
F-test (p-value)	0.157		0.628		0.0169		0.273	
Observations	32 366		15 274		10 948		29 260	
Reform 1991	−0.092	−0.104	−0.054	−0.163**	−0.134	−0.133	−0.059	−0.111
F-test (p-value)	0.0592		0.0443		0.187		0.0544	
Observations	33 124		14 856		10 786		29 913	
Reform 1992	0.042	0.041	−0.014	0.055	−0.018	−0.023	0.005	0.010
F-test (p-value)	0.452		0.499		0.822		0.861	
Observations	33 711		14 682		10 666		30 303	
Reform 1993	−0.170	0.012	−0.166	−0.086	0.208	−0.048	−0.178	−0.003
F-test (p-value)	0.83		0.301		0.626		0.952	
Observations	33 912		14 155		10 521		30 541	
Reform 2005	0.027	−0.046	0.113	−0.069	0.022	−0.120	−0.133	−0.068
F-test (p-value)	0.344		0.373		0.171		0.232	
Observations	39 645		16 209		12 711		29 270	
Joint p-value	0.3594		0.3935		0.3246		0.5073	

Note: All estimates are obtained from a linear RD model with triangular weights using a bandwidth of 6 months before and after each reform. In each panel and for all samples and subsamples, 'F-test (p-value)' refers to the p-value of the F-test that all coefficients are jointly statistically significant. 'Joint p-value' reported at the bottom of the table refers to the p-value of the test that all 16 coefficients across reforms are statistically significant.
$p < 0.05$, $p < 0.01$.

being in the C-suite of the organization, we present estimates for 11 separate years (from 2003 to 2013) for all interventions, except for the 2005 reform for which we have again eight annual estimates from the year after the reform to the end of the sample period.

In the benchmark specification, the time window around every reform is defined to be six months, although we perform a number of sensitivity checks to test the robustness of the results to this assumption.[17] The discontinuity that model (1) exploits arises from the reform being contingent on the birth date of the child. As an example, let us consider the 1992 reform. Mothers of a child born on April 1, 1992 or after were entitled to 35 weeks of fully paid leave, while those with a child born on March 31, 1992 or before could only receive 32 weeks. When estimating the effect of the 1992 reform, it should then be kept in mind that each β_a picks up the impact on y_a of this *marginal* increase in leave. From Table 1, we can see that each intervention extended maternal leave from a minimum of two to a maximum of seven weeks, while the 2005 reform kept the same duration constant for mothers and increased the leave for fathers by one week.

Although it is unlikely that the same woman contributes to the treatment group one year and to the control group the subsequent year, or vice versa, this switch of treatment status could nonetheless occur during the sequence of the seven reforms from 1987 to 1993. For this reason, we performed sensitivity checks in which we drop individuals who switch treatment status. Since such an exclusion does not alter any of the benchmark estimates, the results from that analysis are not presented for the sake of brevity.

We perform a number of tests to validate the RD assumptions. First, we verify that there is no manipulation of the assignment variable x, the child's birth date. Appendix Figure C.3 shows there is no systematic effect of the reforms on the distribution of births around the cutoff, t.[18]

Another check is that parents cannot modify their eligibility status around the cutoff date. Eligibility to maternity leave is essentially driven by the mother's annual earnings in the calendar year prior to the reform. We restrict the sample to women

[17] Essentially, we allow for shorter time windows, from five months down to one month around the introduction of each reform. The estimates from all these different bandwidths confirm our main results. In the Online Appendix, we report some of the results obtained with a bandwidth of three months. Given the large number of results, we cannot show all of them. These results can be obtained upon request.

[18] Dahl et al. (2016) provide additional evidence that x cannot be timed in response to the reform (e.g., randomness of announcement and implementation dates) and discuss birth practices up to the 1992 reform that made it hard for women to postpone induced births and cesarean sections. The same considerations apply to the two later reforms in our study.

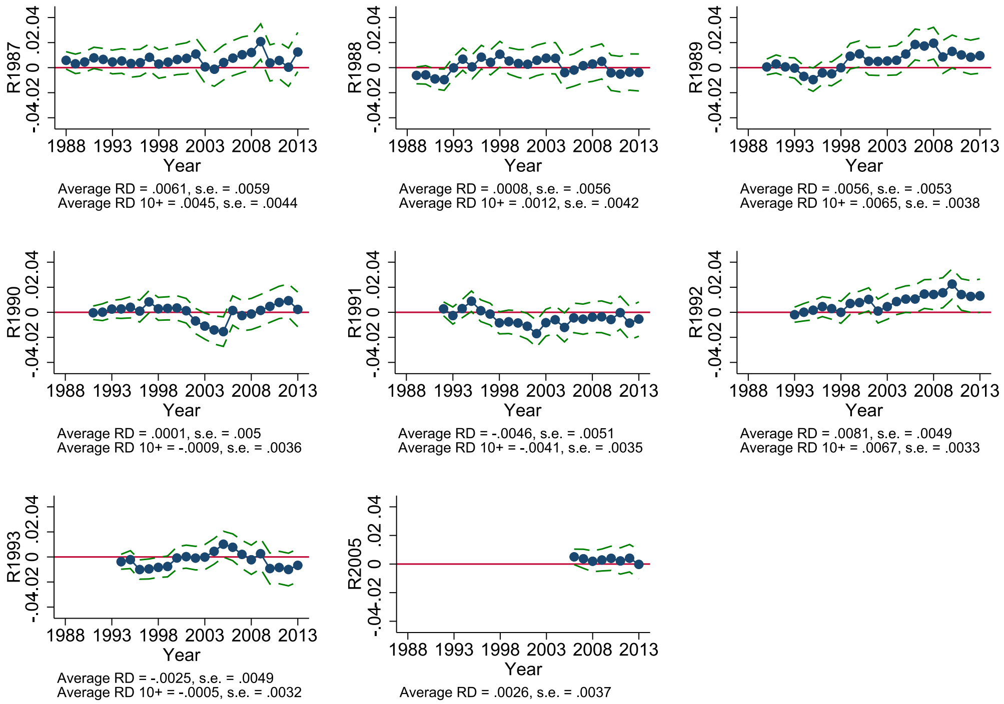

Fig. 4. Mothers' probability of being in the top earnings decile of their firms. ***Note***: Each panel shows the estimated RD coefficients (as dots) for each reform by post-reform calender year (or, equivalently, by child age). The dashed line around the coefficients are the 95% confidence intervals. Estimates are obtained from Eq. (1), using a linear RD model with triangular weights and selecting the sample of mothers whose children were born 6 months before and 6 months after each reform. The sample of analysis is restricted to mothers. At the bottom of each panel, we report the average RD coefficient and its standard error, which are obtained as the weighted sum of the yearly RD coefficients and standard errors, weighted by the number of observations in a given year (or, equivalently, at a given child age) divided by the total number of observations in the entire post-reform period for that specific reform. The outcome variable is a binary indicator of whether a mother is in the top earnings decile within her firm.

who have received labor income in the year prior to childbirth. Imposing a stronger restriction, such as having worked for two consecutive calendar years before birth, does not change our estimates. Furthermore, the distribution of parents' predetermined characteristics may differ around the introduction of the reform if parents can manipulate their child's date of birth or their own eligibility status. We find little evidence that this is the case. Only one of the 16 estimates shown in the first two columns of Table 3 for mother's age at childbirth and education is statistically significant, and we cannot reject the hypothesis that all 16 coefficients are jointly equal to zero (p-value = 0.359). Repeating the exercise for other different subsamples used in the analysis below leads to similar conclusions (see the remaining columns of Table 3). Despite this balance, all our RD regressions control for mother's age and education, as well as municipality fixed effects. Excluding them does not alter the results.

Finally, the estimates in the third column of Table 3 are small and never statistically significantly different from zero, suggesting that none of the reforms had an impact on the timing of first births. Also, as shown in Appendix Table B.2, none of the reforms affected either the spacing between first and second births or the total number of children. This set of results provides strong evidence that fertility outcomes were unchanged by the leave extensions.

5. Main results

5.1. Top pay and leadership jobs

We present our RD results graphically, separately for each reform. Fig. 4 plots the year-specific (or child-age-specific) effect, β_a in Eq. (1), of each reform on the probability that a mother is in the top earnings decile within her firm. We track women from the year following the reform to the end of the sample period, with each dot in the figure representing a different year (or child age) and the dotted lines around the point estimates indicating the 95% confidence intervals. Following Gelman and Imbens (2019), the different f_a functions to the left and right of each reform are linear in the running variable x (month of childbirth) and estimated using triangular weights. Each regression controls for a cubic polynomial in mother's age, years of schooling, and municipality fixed effects. At the bottom of every panel, we report the mean effect of each reform averaged over all the post-reform years, $\sum_a \pi_a \beta_a$, where π_a is a weight given by the number of observations in the year in which the child age is a divided by the total number of observations in the entire post-reform period for that specific reform. We also report the mean effect averaged over the period ten

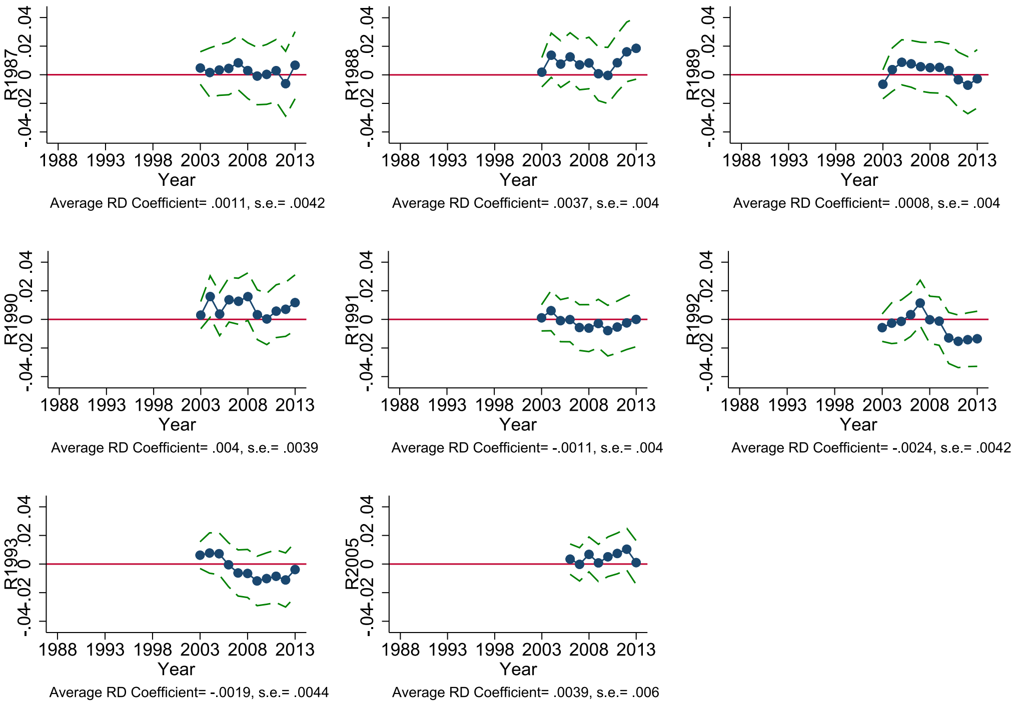

Fig. 5. Mothers' probability of being in top executive or board director posts. ***Note***: The outcome variable is a binary indicator that a mother holds a CEO, CFO, or board director post. For other details, see the note to Fig. 4.

years after each reform (labeled "Average RD 10+"). In this way, we take away the years in which mothers are younger and less likely to be at the top of their organization's pay distribution.

Fig. 4 provides little visual evidence that parental leave extensions affected mothers' chances to be in the top earnings decile of their organization in both the short, medium, and long run. We do find that the 1992 reform increased such chances by about 2 percentage points between 2007 and 2012, when children were 15 to 20 years old. A similar, albeit shorter lived, positive impact emerges after the 1989 reform between 2006 and 2008 when children were in their late teens. The response to the 1990 reform instead was negative between 2003 and 2005. In general however, considering all the reforms together, there is no systematic pattern of results with a sustained (either positive or negative) impact over time. Put differently, none of the reforms contributed to the observed increased representation of mothers in the top echelons of their companies' pay revealed by Fig. 1. At the same time, none of the leave expansions had a negative impact on the mothers' likelihood to reach the top pay decile in their firms. Additional estimates displayed in Online Appendix Figures C.4 and C.5 show that the main results are not sensitive if we narrow the bandwidth around the reform cutoff date from six to three months.

The same evidence emerges when we average out the impact of each extension, over either the whole post-reform period or starting 10 years after every intervention. They are statistically indistinguishable from zero and quantitatively small, with effect sizes ranging between –0.4 and 0.7 percentage points (or, at most, 5% of the baseline probability for a mother to be in the top decile in 1987 and 2% of that in 2013).[19] Using the proportion of mothers in the top earnings-by-age decile as our outcome does not change the results.[20]

Another, more direct way of assessing whether the expansion in parental leave duration affected mothers' chances to break the glass ceiling is to re-estimate the model using the probability for being in the C-suite (CEO, CFO, board chair, or board director) as our new outcome. Fig. 5 presents the results, which are available only from 2003 onwards. This clearly shows that none of the eight parental leave reforms contributed to the increasing trend reported in Fig. 2. Nor did they significantly impact mothers' high-flying careers negatively. The estimates indicate that the average effect size over the available post-reform period are small, going from

[19] To trace out effects on other parts of the earnings distribution, we also look at the effect of the leave extensions on the probability that mothers are in the middle (from the 40th to the 60th percentiles) or in the bottom decile of the earnings distribution within their firm. The results in Online Appendix Figures C.6 and C.7 document that, even in these other segments of the distribution, there is no evidence of a systematic impact of the reforms. Extending maternity leave duration, therefore, had neither an immediate nor a delayed effect on mothers' relative intra-firm pay position, regardless of whether we look at the top, middle, or bottom of the earnings distribution.

[20] In that exercise, we restrict the analysis to women in firms with at least 10 coworkers within the same age group. As a robustness check, we repeated the analysis on mothers in firms with at least 30 coworkers. We also redefined the outcome using 10-year age categories. In all cases, the results are similar and are thus not reported for brevity.

–0.2 to 0.4 percentage points, at most 2% of the baseline probability to be in the C-suite for a mother. If we consider the fraction of women selected as chairs or directors of boards separately from the female share in top executive positions, we find the same null results. We reach the same conclusion also when we analyze the likelihood of mothers holding a managerial occupation (see Appendix Figures C.8 and C.9).

Summing up our benchmark results, we find little support for a short-term reduction in the probability of being a top earner among mothers who faced leave expansions. Similarly, there is no evidence of a long-term increase in the same probability. Even more clearly, mothers did not see their chances to be in the C-suite either compromised or enhanced, even two decades or more after childbirth. This evidence, therefore, convincingly shows that prolonged leave neither favors nor hinders mothers' career success.[21]

In the previous section, we discuss the results of a few tests we performed to validate the RD assumptions. We also consider the possibility of manipulation of the reform within a small window around the cutoff. This could arise if (i) mothers strategically adjust the day of birth (e.g., through Cesarean section) to benefit from longer leave; or (ii) firms choose to compensate employees for longer leave even if they gave birth slightly earlier than the reform eligibility date. The first possibility is rejected by the evidence displayed in Appendix Figure C.3, which shows continuity of the density of births around the reform cutoffs.[22] To address the potential source of endogeneity induced by the second possibility, we conduct a donut-hole RD exercise which checks the sensitivity of our estimates by excluding the observations closest to the cutoff (Cattaneo and Titiunik, 2022; Barreca et al., 2011). Appendix Figures C.10 and C.11 re-estimate model (1) on our top pay and C-suite outcomes, excluding mothers giving birth within one month of the reform dates. These new estimates are similar to the results presented in Figs. 4 and 5.

Confirming that the leave extensions did not induce selection in/out of the labor force is important to the credibility of these null results. We thus ask whether the policy interventions changed human capital accumulation or not. Appendix Figure C.12 shows that none of the reforms affected work experience, enabling mothers to stay attached to the labor market despite the longer (temporary) interruptions of employment. Although this might have allowed mothers to retain part of their firm-specific human capital, it was not enough to support their careers to climb to the top rung of their companies. While this result could be seen as a missed opportunity, it nonetheless indicates that long work interruptions due to maternity leave do not penalize mothers. Similar null estimates emerge also for firm tenure, as documented in Figure C.13 of the Online Appendix.

5.1.1. *Cumulative impact*

One concern with the analysis so far is that each reform only extends leave by 2 to 4 weeks and since we estimate each reform separately, this may capture only marginal changes in maternity leave duration. In other words, each incremental change may be too small to detect any statistically significant effect of the increased leave generosity. There might be nonlinear effects that emerge only when work interruptions are long enough.

To address this statistical power issue, we look at the cumulative impact of all policy changes, using a difference in discontinuity (or diff-in-disc) approach (Grembi et al., 2016). To keep the comparison clean, we exclude the 1993 and 2005 reforms, since these introduced and extended the daddy's quota.[23] This estimator combines the previous RD specification with a difference-in-difference design fitting linear regression functions to the observations distributed within a given distance (in months) on either side of the 1992 and 1987 reforms.[24] It implies estimating the boundary points of four regression functions of y on x, two on both sides of the normalized distance of child's birth from the cutoff, both before 1987 and after 1992. Formally, we restrict the sample to mothers who had a birth within the six-month interval around each of the two reforms and estimate the model

$$y_{it} = \delta_0 + \delta_1 x_{it} + m_i(\gamma_0 + \gamma_1 x_{it}) + \tau_t[\lambda_0 + \lambda_1 x_{it} + m_i(\theta_0 + \theta_1 x_{it})] + \xi_{it}, \tag{2}$$

where m_i is a dummy variable indicating eligibility for mother i to longer maternity leave, τ_t is an indicator for the post-treatment period (i.e., taking value one for the post-1992 period, and zero otherwise), and x_{it} is the normalized distance in months of the child's birth from the relevant reform's eligibility cutoff. The parameter θ_0 is the diff-in-disc estimate, which identifies the effect of the maternity leave extension between 1987 and 1992, as the treatment is $\tau_t m_i$, and corresponds to the change in the effect of the 1992 reform over its 1987 counterpart. We interpret this as the cumulative effect of the incremental leave duration changes pre-1993.[25] Since we do not consider the 2005 reform, we perform the main exercise using data only up to 2004. Extending the analysis beyond 2004, while excluding women who had a child around the 2005 reform or any time after that, does not change our findings.

[21] Benchmarking these and the previous estimates to available estimates in existing studies is difficult, because our key outcomes have not been studied before. Perhaps, the closest comparator can be found in Kleven et al. (2024), who analyze the earnings effects of parental leave changes on mothers in the top quartile of the pre-birth earnings distribution. They find statistically insignificant effects ranging from −0.02% and 0.05% five to 10 years after the two reforms that expanded parental leave duration in Austria in 1990 and 2000 (see their Appendix Figure B.X). For Norway, Dahl et al. (2016) find that the probability that mothers returned to work two years after a reform ranges between –0.007 (s.e. = 0.009) and 0.020 (s.e. = 0.011) in the case of the 1992 and 1989 reforms, respectively (see their Table 3).

[22] Two other papers have found evidence on the strategic timing of births regarding the introduction of paternity leave in Norway, particularly close to the reform date (Cools et al., 2015; Hart et al., 2022).

[23] The inclusion of the 1993 reform in the diff-in-disc framework is straightforward and does not change the results discussed below.

[24] Since we do not have information on board and top executive positions at the time of the introduction of these reforms, the diff-in-disc design cannot be applied to measure cumulative effects on C-suite outcomes.

[25] Although demand-side time-varying factors spurred by the policy introductions, such as statistical discrimination, could affect the difference feature of this estimator, its discontinuity dimension is likely to minimize this potential bias. A more comprehensive analysis of the firms' response goes beyond the scope of this paper and is left for future research.

Table 4
Difference-in-discontinuities estimates, 1987–2004.

	Probability of being in top within-firm earnings decile					
	All mothers (a)	Excluding 1987 and 1992 (b)	First-time Mothers (c)	Only children (d)	Large firms (e)	Movers (f)
θ_0	0.003	0.002	0.004	−0.006	0.006	0.002
	(0.004)	(0.004)	(0.005)	(0.007)	(0.007)	(0.004)
Observations	744,721	683,133	328,657	118,379	241,957	676,473
Outcome mean, 2004	0.224	0.223	0.224	0.303	0.186	0.234
Outcome mean, 2013	0.277	0.276	0.277	0.338	0.215	0.288

Note: The time period under analysis is restricted to 1987–2004, in order to eliminate confounding by the extension of paternity leave in 2005. In the estimation of Eq. (2), we use a linear control function of the running variable on each side of the cutoff with a bandwidth of 6 months. Controls include a cubic function of age and years of education measured in the pre-reform year, as well as municipality fixed effects. Robust standard errors are clustered at the firm level. Outcome means in the last two rows report the proportion of mothers among all employees within the top decile of their firms' earnings distribution in 2004 and 2013.

Table 5
Mothers' probability of being in the within-firm top earnings decile and in the C-suite, pooled sample.

Years since reform	1	5	10	15	20	25
Outcome: Probability of being in the top earnings decile						
RD coefficient	−0.0001	0.0009	0.003	0.003	0.007*	−0.003
	(0.001)	(0.002)	(0.002)	(0.002)	(0.004)	(0.005)
Outcome Mean	0.088	0.134	0.169	0.237	0.286	0.306
Outcome: Probability of being in the C-suite						
RD coefficient				0.005*	0.0006	0.007
				(0.003)	(0.004)	(0.008)
Outcome mean				0.265	0.320	0.341
Observations	149,808	140,836	145,858	155,121	154,991	40,816

Note: Estimates are obtained on the pooled sample of the reform-specific samples of the first six reforms, 1987–1992.

The baseline diff-in-disc result in column (a) of Table 4 is unambiguous. The impact of extending maternity leave duration from 18 to 35 weeks with full income replacement on the probability of being in the within-firm top earnings decile is economically negligible (with an increase of less than 1.5% over the outcome mean at the end of the period) and statistically insignificant. The same emerges in column (b), where we exclude women who were exposed to both the 1987 and 1992 reforms and find an insignificant size effect of less than 1% over the outcome mean. The small expansions induced by each reform, therefore, do not seem to pose a problem of statistical power. Differently from Ruhm (1998), Ejrnæs and Kunze (2013), and Bailey et al. (2019), this finding does not lend support to the view that longer maternity leaves induce women to accumulate less work experience and, as a result, face lower labor market earnings and a lower likelihood of being in the upper pay echelon of their firms. Likewise, the same estimates do not lend support to the idea that mothers would benefit from prolonged leaves in terms of deferred wage progression.

A different statistical power issue may stem from the concern that the baseline reform-specific estimates rely on small samples. This is unlikely, because we use the universe of births around each reform. Nonetheless, we perform an additional exercise which increases our sample size considerably. That is, we first stack the data for the 1987–1992 reforms into one sample of more than 140,000 births as opposed to an average sample size of about 30,000 for each reform in isolation. We then estimate Eq. (1) on this pooled sample.

Table 5 shows the estimates for the probability of being in the top decile and the probability of being in the C-suite by year since reform. Referring to 1, 5, 10, 15, 20, and 25 years since reform (or, equivalently, child's age), the results offer a complete picture of short- and long-run effects. Notice that the estimates at 25 years can rely only on two reforms (the first two ones), which explains the slightly larger confidence intervals, whereas up to 20 years all policy interventions contribute to the estimation, except the 2005 expansion. Both sets of estimates confirm our previous results that the maternal leave extensions had no impact on either outcome. Moreover, for both outcomes, the average RD coefficients are generally small and statistically insignificant. We do observe a positive impact of 0.7 percentage points on mothers' chances to be in the top earnings decile 20 years after the reform enactment, but this effect is isolated, modest in size (representing an increase of about 3% over the sample mean), and statistically significant only at the 10% level. Also, it is not mirrored by success in the boardroom. Similar results emerge if in the estimation sample we also include the births around the 1993 reform, or if we use a shorter bandwidth of three months around each reform, or if we consider the sample of first-time mothers.

Reiterating the previous benchmark results, we find that cumulating the effect of the first six reforms, which jointly prolonged leave from 18 to 35 weeks, neither improved the relative pay of mothers within their organizations, nor promoted them to positions of leadership in the corporate world both in the short and the long run. The same null result emerges when we combine each reform-specific sample into one larger sample.

5.2. Heterogeneity

Although we find no evidence of an impact on mothers' top pay and leadership positions, parental leave expansions might have affected women differently depending on their level of education, the number of children they had at the policy onset, or the type of firm or industry in which they worked. In what follows, we explore these possibilities.

5.2.1. Education

It is possible that better educated women are in high-pay jobs even before childbirth. If employers value the stability of job-to-worker matches, better educated mothers may use longer maternity leaves to climb to, or remain at, the top of their organizations' pay scale even after birth. The cross-country evidence presented by Olivetti and Petrongolo (2017) shows instead that longer parental leave is associated with wider earnings gaps among college-educated mothers. We thus repeat the previous analysis focusing only on women with university or higher qualifications for the probability of being in the top earnings decile intra-firm. The results in Appendix Figure C.14 confirm that even for college-educated women the extensions in maternity leave did not contribute, either positively or negatively, to the growth in female representation at the top of the intra-firm pay distribution.

5.2.2. Parity

We find that the leave expansions have no impact on birth timing.[26] We also find no evidence of any effect on mother's age at first birth and on spacing between first and second births and completed fertility (see Appendix Table B.2). Nonetheless, as shown by Lalive and Zweimüller (2009), there might be effect heterogeneity of the reforms by birth order. Larger families may impose higher career costs on the main carer, usually the mother, even when women have high earnings potential or are highly educated (Francesconi, 2002; Guryan et al., 2008; Adda et al., 2017). Some studies emphasize the role of scale effects, and argue that the marginal cost of second (and subsequent) children is usually less than that of the first child (Browning, 1992).

We therefore re-estimate Eq. (1) for mothers who are exposed to the reforms when giving birth to their first child separately from those who have their second child. The results do not change when we consider higher parity households (third or fourth child). The estimates on the probability of being in the top within-firm earnings decile among first-time mothers are shown in Appendix Figure C.15.

We find that the 1989 reform pushed this probability up by approximately 2 percentage points from 2006 to 2011, when first-born children were between 17 and 22 years old. The average impact over the post-reform period however is smaller, around 1 percentage point, and statistically insignificant. The responses to all the other reforms are similar to those found for all mothers, indicating no systematic effect of paid leave on mothers' chances of being in top pay jobs in their companies.[27]

The same null results across all outcomes emerge also for second-time mothers and for mothers of only-children, which are not reported for brevity. The findings for mothers of only children as well as first-time mothers are also confirmed by the diff-in-disc estimates shown in columns (c) and (d) of Table 4, respectively. Taking stock of all these results, we conclude that the extensions to paid leave did little to affect either first- or second-time mothers' chances of entering, or staying in, the top pay decile of their organizations.

5.2.3. Firm size

Career opportunities may vary substantially with firm size, with wage spreads rising with the number of workers in order to compensate for the increased competition for higher-ranked jobs and with larger firms having better defined hierarchies and internal labor market structures (Gabaix and Landier, 2008; Gayle et al., 2015; Huitfeldt et al., 2023). We distinguish three sets of firms based on the number of employees: small (4–9), medium (10–99), and large (100 or more).[28]

The results in Appendix Figures C.19–C.22 show that the 1992 reform raised the probability to be in the top earnings decile for mothers in large firms, from 2008 to 2013. The 1990 reform instead reduced this probability for women employed in medium-size firms between 2003 and 2005. We cannot detect any other significant effect. The aggregate effects are never statistically significant, irrespective of firm size and of the reform we focus on, and so is the cumulative effect for large firms shown in column (e) of Table 4. We also find no significant heterogeneous impact by firm size on leadership positions. Regardless of the complexity of the internal labor market which may be associated with firm size, there is therefore no evidence that the expansions to parental leave influenced mothers' chances to reach their companies' upper earnings decile or to be represented in their executive boards.

5.2.4. Industry

Another dimension of heterogeneity through which there might be strong gender wage differentials is the type of industry in which men and women work. Parental leave expansions could magnify or attenuate such differentials.

[26] This is in line with Dahl et al. (2016) who found this result up to the 1992 reform.

[27] We also find no impact of any of the extensions on first-time mothers' chances of being in the middle or bottom of the intra-firm earnings distribution. Nor can we detect any significant effect on their chances of filling positions of leadership in their organizations. See Appendix Figures C.16–C.18.

[28] Using a sample of approximately 2000 Norwegian firms, Huitfeldt et al. (2023) focus their analysis on "large" firms, which are defined as those with at least 30 employees. Our results remain qualitatively unaltered if we redefine large firms using this alternative definition. Notice also that, because of sample size issues, the analysis cannot be performed on small firm workers. Aggregating them to workers in medium size firms does not change our estimates.

Goldin (2014) and Goldin and Katz (2016) argue that the extent of temporal job flexibility, without substantial wage penalties, largely depends on industry- or occupation-specific technological features, including characteristics which determine the need for workers to be available at particular (nonstandard) times, the degree of close substitutability among workers, the flexibility of the job with regard to scheduling, and the need for an employee to keep in touch with other workers (above and/or below their position in their own organization) or specific groups, such as clients and stakeholders. Goldin (2014) also provides evidence that there is a strong negative relationship between earnings gender gaps and the elasticity of earnings with respect to hours worked. Larger elasticities correlate positively with the above mentioned technological characteristics when these imply more time pressure. Larger elasticities are also typically observed among workers in business and financial services, while lower elasticities emerge in technology, science and health occupations.

To match those industrial sectors in our data, we use detailed information from the 5-digit 2007 Standard Industrial Classification, and define three separate groups of workers, one in finance (including banking and insurance), another in health (including physicians and dentists) and the last one in business and technology (including R&D, market research, advertisement, and business consultancy). These can be reliably constructed from 1998 onwards. We repeated our benchmark analysis for each industrial group separately. The estimates reported in Appendix Figures C.23–C.25 show that none of the leave reforms had an impact on mothers' chances to be in the top earnings decile of their companies, irrespective of the grouping of industries.

5.3. Channels

Prolonged maternity leaves might have influenced mothers' labor market performance in ways other than through pay or executive leadership, e.g., through hours of work, promotion opportunities, or job mobility. In what follows, we explore such mechanisms.

5.3.1. Hours of work

As emphasized by Goldin (2014) and Goldin and Katz (2016), some positions have a highly convex pay structure with regard to hours worked, requiring a high degree of workplace commitment and little flexibility to combine work and family life. Such positions are generally held by highly skilled workers at the upper end of the earnings distribution. After work, women may also be "downgraded" through shifts into part-time employment as documented for Britain by Manning and Petrongolo (2008). We thus ask if top-earning mothers eligible for longer leave were more likely to change their hours worked.

The results in Appendix Figure C.26 show that women's hours are unaffected by the parental leave expansion policies, soon after birth as well as in the longer run. Repeating the exercise for all mothers, and not just for those at the top tier of pay within their companies, leads to the same null result (see Appendix Figure C.27). The reforms, therefore, did not alter the strong time bind that locks mothers to jobs, regardless of the pay rank within their companies.

Combined with the estimates on work experience and firm tenure mentioned in Section 5.1, these findings confirm that the eight reforms under study had virtually no influence on mothers' human capital investment, in the short or the long run.

5.3.2. Promotions

Male–female wage gaps, especially at the top of an organization, may be driven by differential employer promotion standards due to gender differences in the probability of leaving — or taking career interruptions from — the firm (Lazear and Rosen, 1990). As the cost to employers of job interruptions is greater for workers in high-level jobs than in the low-level jobs, given ability, males are promoted to high-level jobs over females who are equally productive in low-level jobs. Longer breaks arising from the leave extension reforms will then imply even lower promotion probabilities for mothers at the upper end of the earnings distribution.

Most of the empirical literature has focused on gender differences in promotions.[29] Antecol et al. (2018) is the only study to date that explores the impact of a family policy on promotions by gender. They find that gender-neutral tenure clock stopping policies introduced in the top-50 US departments of economics (in which untenured assistant professors are allowed to stop their tenure clock for one year after childbirth) decrease the probability that a female assistant professor gets tenure where she was initially hired while male tenure chances rise. There is, however, no direct evidence of the impact of parental leave policies on promotions.

To analyze this relationship, we use the unique data described in Section 3 (for more details, see Kunze and Miller (2017)). The results reported in Appendix Figure C.28 document that mothers experienced neither greater nor lower chances of promotion in response to the reforms. This null result is also confirmed in Appendix Table B.3 by the cumulative effect obtained from the diff-in-disc estimator. The same findings also emerge when we consider promotions associated with a change of employers (see Appendix Figure C.29). We also examined the possibility that the reforms affected mothers' chances to be in the top earnings decile within their firms if they were internally promoted at least once after childbirth. The estimates (not reported for brevity) show no evidence of an impact.

5.3.3. Firm mobility

In addition to within-firm mobility, mobility across firms is considered to be a major contributor to wage growth over workers' careers (Postel-Vinay and Robin, 2002; Del Bono and Vuri, 2011; Bagger et al., 2014; Adda and Dustmann, 2023). Card et al. (2016)

[29] The results from this literature are mixed. Some find lower rates of promotion for women than for men with similar observed characteristics (McCue, 1996; Cassidy et al., 2016), others find the opposite (Booth et al., 2003; Gayle et al., 2012), and still others find no gender differences (Giuliano et al., 2011).

explore this channel in combination with the possibility that women are offered systematically lower wages by their employers to study gender disparities in the Portuguese labor market. They find that both channels explain approximately one-fifth of the cross-sectional gender wage gap.

We stratify the sample into two groups of mothers, those who stayed with the same employer and those who moved at least once from one employer to another after childbirth over the sample period. The estimates in Appendix Figures C.30 and C.31 refer to movers. Overall, we find no evidence that the leave extensions affected mothers' probability of reaching the upper pay decile of their company if they moved across firms. The 1992 reform did raise this probability by about 2 percentage points from 2007 to 2011, and so did the 1989 reform between 2006 and 2008, while the 1990 and 1991 extensions led to a reduction between 2003 and 2005 and between 2001 and 2003, respectively.

The general reading of this evidence is that each of the eight reforms did not contribute much to the increased representation of mothers in the upper echelons of their firms' earnings, even for women who moved across firms. The same can be said for mothers' chances to be in their firms' board or top executive positions. A null average impact result is confirmed for both outcomes across all reforms. There is also no evidence of a cumulative impact, as shown in column (f) of Table 4. The same set of null results emerges among mothers who stayed with the same employer over the entire period (not reported for the sake of brevity).

5.3.4. Conditional transitions across the income distribution

Mobility to top earnings may be hard, especially for workers who start low in the pecking order (Bagger et al., 2014). To account for this possibility, we examine transitions to the top earnings decile from specific parts of the earnings distribution. In particular, we analyze the impact on the likelihood that mothers have to reach the top pay decile in their own firms in a given year if they were in the second highest decile in the year before each reform. The estimates shown in Appendix Figure C.32 reveal no effect of the reforms on this likelihood. Repeating the analysis when we condition on other starting positions in the intra-firm earnings distribution leads to the same conclusion (not reported for brevity). Thus, the reforms did not affect mothers' probability of reaching the top echelons of their companies' pay, irrespective of their initial position in the firm's earnings distribution.

Putting together all our results on within- and between-firm mobility, we infer that the expansions in parental leave seem to have played no role in mothers' search and matching decisions.

5.4. Fathers

Norway offers an interesting case because it introduced paid leave earmarked for fathers since 1993 (see Table 1). This means that the couple foregoes his quota, should the father not use this provision. Two of the eight reforms under analysis included a paternity quota. The 1993 reform gave fathers a four-week quota for the first time. This was extended to five weeks with the 2005 reform.

As evidenced in Fig. 6, none of the pre-1993 reforms had an impact on the probability that fathers be in the top earnings decile within their firms. Although this may be unsurprising (because such reforms affected only mothers), it clearly suggests that those reforms generated no wage spillover for fathers. But even the two reforms that provided paid leave for fathers did not affect intra-firm male earnings success.[30] The lack of an impact emerges also in the middle and at the bottom end of the fathers' earnings distribution (see Appendix Figures C.33 and C.34).

Similarly, none of the reforms had an impact (either positive or negative) on fathers' chances to be a member of company boards or in top executive posts, as presented in Appendix Figure C.35. This is true also for the likelihood of being in a managerial occupation, except for the 1991 and 2005 reforms, which respectively reduced and increased that probability by 2–4 percentage points, albeit only temporarily (see Appendix Figure C.36). Taken all together, therefore, there is little evidence of a systematic impact that an increased generosity in parental leave had on fathers' intra-firm top pay or leadership jobs.[31]

6. Intra-household gender pay inequality

None of the eight reforms influenced the success of mothers (or fathers) to reach top positions within their organizations. Extended parental leave nonetheless could have affected women's economic progress in other ways. For instance, it could have changed the relative earnings power of mothers with respect to their partners'. Proponents of government-mandated paid parental leave extensions expected these reforms to promote more intra-household gender equality (Vollset, 2011; Dahl et al., 2016), strengthening the bargaining position of mothers (Doepke and Kindermann, 2019). Strikingly, within-household inequality has hardly been analyzed before in conjunction with parental leave policies.[32]

To perform our analysis we use full-time equivalent monthly wages which adjust for differences in hours worked, so that hours of work cannot play any role here. The estimates are displayed in Fig. 7. This refers to women who are in the top decile of the full-time equivalent monthly wage distribution within their companies compared to their partners. A positive estimate captures a

[30] This result does not change even if the ITT estimates are adjusted by the change in paternal leave take-up around the two reforms.

[31] Despite these null results, we cannot exclude that the daddy's quotas might have had an impact on beliefs, yielding counter-stereotypical behavior, as postulated by Bertrand (2020). This is left for exploration in future research.

[32] One notable exception is Hart et al. (2022), which shows that the 2009 paternity leave extension in Norway has not altered relative earnings of couples. Dahl et al. (2016) also analyze the ratio of male to female income annuities in the longer run (i.e., 14 years after each of the 1987–1992 reforms). Their estimates range from –0.003 (s.e. = 0.004) and 0.006 (s.e. = 0.004) for the 1989 and 1991 reform, respectively (see their Table 4).

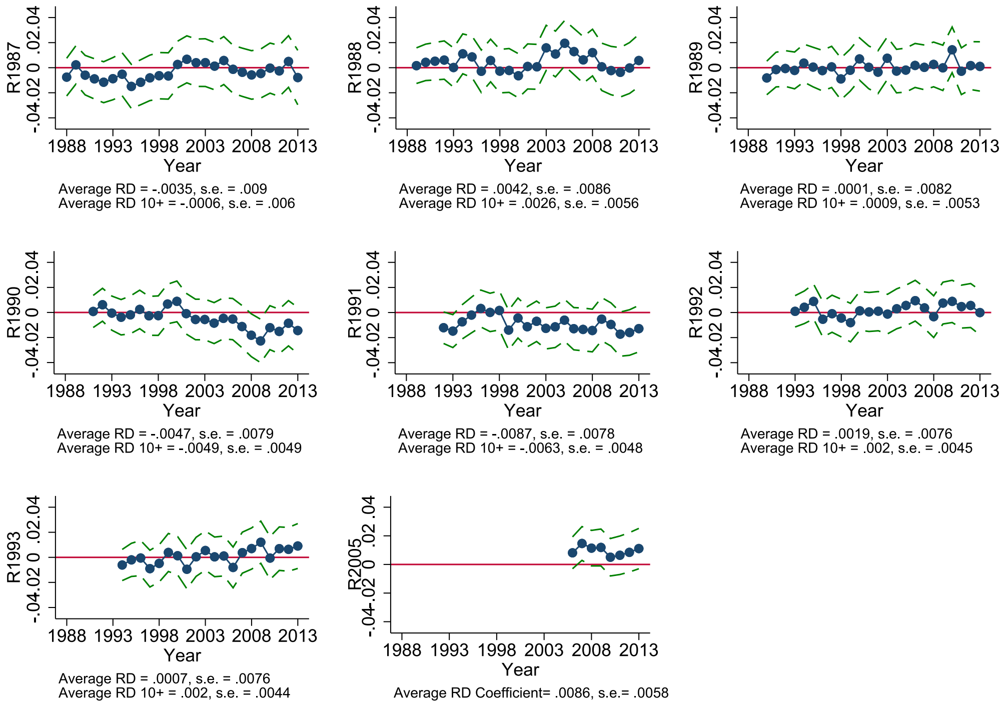

Fig. 6. Fathers' probability of being in the top earnings decile of their firms. *Note*: The outcome variable is a binary indicator that a father is in the top earnings decile within firm. The sample of analysis is restricted to fathers. For other details, see the note to Fig. 4.

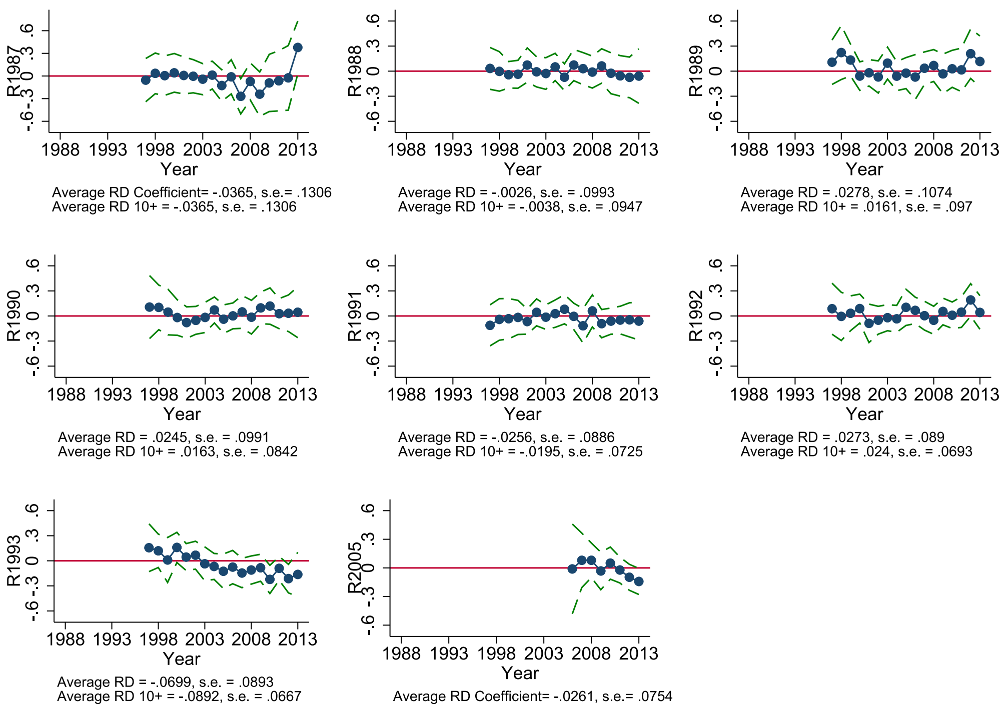

Fig. 7. Within-household gender wage gap at the top decile. *Note*: The outcome variable is the gender wage gap between women at the top earnings decile of their firms and their partners, computed using full-time equivalent monthly wages. For other details, see the note to Fig. 4.

reform-led wage improvement in favor of mothers, while a negative estimate reflects a worsening with respect to their partner's wage. Overall, we find no impact of the leave extensions. The estimates are typically small and statistically indistinguishable from zero, and this is clearly documented by the aggregate impacts reported at the bottom of each panel. Remarkably, also 10 years after every intervention the effect is zero. As shown in Appendix Figure C.37, the same null results emerge if we consider all women, and not just those in the top decile of their firms' earnings distribution.

We also detect no changes in the intra-household gap when we look at annual earnings for both mothers in the top decile and all mothers (see Appendix Figures C.38 and C.39). The null result on top earners is unequivocal about the neutrality of the leave extensions to change the intra-household pay gap, despite the considerable progress observed in Fig. 3 among top female earners since the late 1990s.

7. Conclusion

This paper presents new evidence on the under-representation of women and mothers in leadership positions over the period 1983–2013 in Norway. To identify leaders, we use a number of alternative measures, and more specifically individuals in the upper decile of their firms' earnings distribution and individuals in C-suites. Although more women and mothers have gradually entered leadership positions, two-thirds of top earners in 2013 were men. Of the one-third who were women, about 85% were mothers. The figures for top executives are similar.

We then provide what we believe is the first comprehensive assessment of whether shorter or longer parental leave – in a regime of fully paid job-protected leave – harm or sustain mothers' economic progress to the top of the career ladder. Our analysis follows women up to a quarter of a century after childbirth. We use employer–employee matched register data that allow us to take advantage of eight parental leave reforms, which more than doubled maternal leave from 18 weeks in 1986 to 38 in 1993 and introduced a paternity quota of 4 weeks in 1993 and extended it to 5 in 2005. We employ a regression-discontinuity design, perform several tests to validate the RD assumptions, and account for potential statistical power issues looking at cumulative impacts.

We emphasize six new substantive results. First, both the expansion of paid maternity leave from 18 to 38 weeks and the introduction of a quota of leave reserved to fathers had no effect on mothers' economic progress to reach a top position in the short, medium or long run.

Second, the reforms neither fostered nor compromised mothers' economic empowerment, as we find no evidence of a significant change in the likelihood of entering the C-suite of their organization.

Third, the expansions had no differential effect across several subgroups by maternal education, number of children (or child parity), firm size, and industry. This includes high achievers already before birth who may be more likely to reach the top positions later during the careers, that is highly educated women, high performers, and women working in female-dominated sectors, such as health.

Fourth, we extend the literature by going a step further and asking whether other channels, which are relevant to career progression, may have played a role in such a way that the net effect on reaching top-pay jobs and executive positions averages out to zero. Estimating the effects of the reforms on work experience, firm tenure, hours worked, internal promotions, and firm mobility shows no significant results. These findings indicate a likely limited role played by human capital considerations in explaining mothers' progress as a direct result of the parental leave expansions. They also suggest a limited role played by labor market imperfections, through search frictions or difficulties in replacing high flyers during or after leave usage.

Fifth, none of the eight policy interventions, including those that introduced paternity leave, had any impact on fathers' pay, and the presence of daddy's quotas did not affect mother's economic position, whether negatively or positively. The large asymmetry in leave duration between mothers and fathers, which assumes women as the main carer, may be important and requires further research. Sixth, the leave reforms had no effect on the gender pay differentials between mothers in top positions and their partners, the fathers.

Taken together, our results suggest that either short or long paid parental leave does not help mothers to break the glass ceiling; but it also does not hinder their chances of success. The positive trend in mothers' entry into top pay and leadership positions happened despite the leave extensions. An interesting dimension, which has been excluded from the current analysis and would complement our policy evaluation, is on firms' responses to long paid parental leave. An emerging literature has started to document that firms respond by recruiting temporary replacements and by relying on increased coworkers' hours, suggesting only small overall employment effects on firms through parental leave (Ginja et al., 2023) or births (Brenøe et al., 2024). Other important firm-level margins seem to be relevant, such as wages, profits, investment, and employment composition.

Data availability

The authors do not have permission to share data.

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Appendix A. Supplementary data

Supplementary material related to this article can be found online at https://doi.org/10.1016/j.euroecorev.2024.104845.

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